

that prominences at the Sun's brink are frequently recorded as dark flocculi when photographed in projection against the disc.

The possible effects of anomalous dispersion were discussed, and photographs were exhibited of the same region of the Sun, taken simultaneously with light from the red and violet edges of $H\alpha$. The similarity of these photographs apparently indicates that anomalous dispersion is not the prime factor in producing the hydrogen flocculi. Certain minor differences suggest, however, that it may perhaps play a secondary part in modifying their form.

On Ancient Eclipses. By P. H. Cowell, M.A., F.R.S.

I have calculated the twenty-one eclipses of which a list was given by Mr. Fotheringham, *M.N.*, lxix. p. 29. In the light of Ginzel's calculations and of the constant doubt as to the exact place of observation, it was to be expected that no formulæ would render all these eclipses central at the places named by Mr. Fotheringham. The best authenticated eclipse seems to be that of +484 at Athens, which is rendered total by both Professor Newcomb's formulæ and my own, exact centrality corresponding approximately to the mean of our formulæ. In addition to the eclipse of +484, my formulæ satisfy the eclipses of -187 (Rome), +218 (Rome), +393 (Constantinople) well; Professor Newcomb's satisfy -393 (Chæronæa), +447 (Chiaves) well. My formulæ satisfy +186 (Rome) rather better than Professor Newcomb's, while for +334 (Sicily) his formulæ satisfy rather better than mine. There is nothing noteworthy in the records except that in -393 totality is expressly denied.

Mr. Fotheringham's last paper is most interesting, chiefly because he bases conclusions on the eclipse of -1062 and the eclipse of Hipparchus only, with little or no stress laid on confirmation arising from other records. That is to say, that in the opinion of a historian competent to judge, these two records are intrinsically of a very high value. My belief that the eclipse of -1062 at Babylon is trustworthy was mainly based on its agreement with several others, though I fully recognised that intrinsically it was among the best that I used. I naturally welcome Mr. Fotheringham's attitude towards this eclipse. As to the eclipse of Hipparchus, we may, it seems, fairly accept Mr. Fotheringham's verdict that at some time between the foundation of Alexandria and the death of Hipparchus an eclipse occurred, which requires to be identified with the record. As far as approximate totality at the Hellespont is concerned, Professor Newcomb's formulæ can be reconciled with the identifications -189 March 14, -182 October 19, -173 October 10, and -128 November 20. My formulæ can be reconciled with the identifications -309 August 15, -173 October 10, -103 July 19. All eclipses total at the Hellespont must be large partial eclipses at Alexandria, but undoubtedly, of those above

mentioned, — 128 November 20 is the only one where the conditions are such that, if there was totality at the Hellespont, a good estimate of the magnitude at Alexandria would be four-fifths. In particular, in order to identify this eclipse with — 309 August 15, or — 103 July 19, we have to suppose that the estimated magnitude was a digit too large; if with — 173 October 10, three-quarters of a digit too small. It must, however, be borne in mind that any formulæ whatever lead to the conclusion that in lunar eclipses magnitudes are occasionally a digit wrong; and further (though I do not agree) that Professor Newcomb has stated that the probable error of a magnitude of a lunar eclipse is more than one-tenth part of the Moon's diameter (*M.N.*, lxvi. p. 472).

Note on the value of the obliquity used in the conversion of the Moon's ecliptic into equatorial coordinates. By A. M. W. Downing, D.Sc., F.R.S.

The point having been raised as to the value of the obliquity adopted in the *Nautical Almanac* calculations for the conversion of the Moon's longitude and latitude into the corresponding right ascension and declination, it may be advisable to state specifically what values have been adopted at different periods since the introduction of Hansen's Tables into the *Nautical Almanac*. This seems to be the more desirable as Hansen, at the end of his tables, gives facilities for the calculation of R.A. and Dec., thus suggesting the possibility that these subsidiary tables may have been used in the conversion throughout the whole period. The following statement will, I hope, obviate any further possible misunderstanding on the subject.

Period.	Authority for Obliquity.
1862–1874	Hansen.
1875–1900	Le Verrier.
1901 onwards	Newcomb.

1909 June 9.